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ABSTRACT

In view of the apparent successes achieved with Labovian quantitative methods in the analysis of phonological variation, it is not surprising to find these techniques being extended to include the study of syntax. Sankoff suggests that the extension of probabilistic considerations from phonology to syntax is not a conceptually difficult jump. However, this optimism is premature. An analogous view of syntactic variation is incoherent; what is meant by the notion of syntactic variation is a moot point. No sociolinguistic or syntactic theory exists that is sufficiently well articulated and restricted to deal with the problem of variation in syntax. The most serious issue raised by the problem of syntactic variation concerns the kind of semantic/pragmatic theory on which the foundations of an integrative sociolinguistic theory should be based. Possible approaches to accounting for this variation include seeing the form of grammatical structure through the relationship between (1) the logical structure of utterances and surface syntactic form; (2) perceptual processing and syntactic process; or (3) conversational organization/interaction and syntactic structure. The third approach has the advantage of allowing a perspective on variation in acquisitional and post-creole continua as well as on both diachronic and synchronic variation. (Author/MSE)

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ON THE PROBLEM OF SYNTACTIC VARIATION: A REPLY TO BEATRIZ LAVANDERA AND WILLIAM LABOV

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ON THE PROBLEM OF SYNTACTIC VARIATION: A REPLY TO BEATRIZ LAVANDERA AND WILLIAM LABOV

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Abstract

In view of the apparent successes achieved with Labovian quantitative methods in the analysis of phonological variation (cf. for example, Labov 1966), it is not surprising to find these techniques being extended to include the study of syntax. Sankoff (1973), for example, suggests that the extension of probabilistic considerations from phonology to syntax is not a conceptually difficult jump. In my opinion, however, Sankoff's optimism is premature. An analogous view of syntactic variation is incoherent; it is a moot point what one means by the notion 'syntactic variation.' We simply do not have a sociolinguistic (nor syntactic) theory which is sufficiently well articulated and restricted to deal with the problem of variation in syntax. Perhaps the most serious issue which the problem of syntactic variation raises concerns the kind of semantic/pragmatic theory upon which the foundations of an integrative sociolinguistic theory should be based.

On the nature of syntactic variation

It has been some time now since Sankoff (1973) first proposed that the scope of the study of language variation should include syntax and semantics. In her analysis of complementizer *que* deletion in Montreal French she demonstrated how Labovian quantitative methods might be extended to deal with variability in levels of the grammar above the phonological. She claims straightforwardly (1973:58):

> The extension of probabilistic considerations from phonology to syntax is not a conceptually difficult jump... It seems clear to us that in the increasing number of situations which have been studied... underlying probabilities are consistently and systematically patterned according to internal (linguistic) and external social and stylistic constraints. There is no reason not to expect similar patterning elsewhere in the grammar.

Sankoff's main purpose was to provide a demonstration of such an extension without giving much thought to the question of whether the nature of syntactic variation is sufficiently similar to that which takes place at the phonological level to justify such a wholesale transfer of method. Similarly, Rickford (1975), who has followed Sankoff's lead, is more concerned with the methods used in the collection of syntactic data, than with the larger, and in my opinion more important, issue of whether sociolinguistic methods are applicable in all respects to syntactic problems. The answers to this question have important implications in a number of areas as I propose to show; most significantly perhaps, the study of syntactic variation raises a dispute about the kind of semantic/pragmatic theory upon which the foundations of an integrative sociolinguistic theory should be based.

Lavandera (1978) has recently argued quite convincingly, I think, that one important methodological tool, the (socio-) linguistic variable, cannot be easily extended to the analysis of syntactic variation. It is however important to note at this stage, as Lavandera herself is careful to point out, that her argument is not where variation stops (since it is an empirical issue whether syntax shows internally as well as externally conditioned variation), but where the linguistic variable stops, i.e. ceases to be a useful or meaningful concept. It is also an empirical (and interesting) issue whether syntax is more resistant to variation than, say, either morphology or phonology. Hudson(1980:46) quite rightly remarks that examples of syntactic differences within a 'language-sized' variety are less frequently cited in the sociolinguistic literature than differences in either pronunciation or morphology. This, in his opinion (and I would agree), requires an explanation.

He suggests a number of reasons:

1. Syntactic differences may be more difficult to study because they occur infrequently and are hard to elicit;
2. There are relatively few syntactic items (or constructions), so that even if the same proportion of these varied the result would still be a smaller number by comparison with the number of phonological variables; and
3. The greater tendency to uniformity in syntax may be an artefact of the process of standardization.

Hudson's first two points are methodological, i.e. assuming there is variation, how can one find 'enough' of it to investigate (cf. Rickford's discussion of this aspect of the problem); the third point is a difficult one. Hudson in fact thinks there is evidence for the view that people tend to suppress alternatives in syntax, while they positively seek them in vocabulary. In this connection he cites the diffusion of syntactic features across language boundaries (cf. for example, Nadkarni 1975). Perhaps one of the most striking instances of the areal diffusion of syntactic features is the case discussed by Gumperz and Wilson (1971) in Kupwar, where the villagers speak three languages, Marathi, Urdu and Kannada. Despite the fact that the languages have co-existed for centuries in this intimate contact situation, they are nearly completely distinct in vocabulary; but, as far as syntax is concerned, they have become much more similar in Kupwar than elsewhere. This is true to such an extent that Gumperz and Wilson claim the three languages now share a common syntactic base.

I am not sure that these examples support Hudson's hypothesis that alternatives in syntax tend to be supressed as markers of social differences, while vocabulary and pronunciation differences tend to be favored. For one thing, these cases of 'syntactic diffusion' in contact situations seem to be more unusual than Hudson implies. Generally, it seems to be true that grammatical and syntactic features diffuse across linguistic boundaries less easily than vocabulary items and phones (cf. for example, Haugen 1950 and Weinreich 1974:14-36). It is therefore usually regarded as a sign of intimate and prolonged contact when one finds shared syntactic features in these kinds of situations. I suspect there are many more instances of language contact where syntactic differences between two or more languages have not been suppressed. For example, Pousada and Poplack (forthcoming) have recently argued that despite heavy contact with English, the grammatical integrity of Puerto Rican Spanish in New York City is maintained, at least at the deep structure level. And Sankoff and Poplack (1979) suggest that code-switching must be a surface structure phenomenon since the basic qualitative and quantitative integrity of both Spanish and English is maintained. That is, the parts of sentences which are English have the characteristic canonical and favored sentence structures of English; and those which are Spanish retain the favored Spanish patterns. They present impressive quantitative data to demonstrate that code-switching

takes place only at certain predictable points where the syntactic structures of English and Spanish allow their respective autonomies to be maintained. This finding of course, raises a number of interesting questions and has important implications for linguistic theory, which Sankoff and Poplack do not pursue.

For example, is it the case that languages which are not very similar will not be code-switched frequently because the morpho-syntactic structures of the languages involved 'allow' fewer points at which switches can be initiated without violating certain syntactic constraints in one or the other language? In multilingual communities where typologically different languages are in use and where code-switching is infrequent, can one discover whether the latter reflects linguistic or social distance, or both? Or, indeed, have community norms about code-switching, if they exist, arisen by dint of linguistic pressure? To take another problematic case, the morpho-syntactic convergence of Chipewyan, Cree, French and English at Fort Chipewyan, Alberta as described by Scollon (1979:225), closely parallels that reported for Kupwar. However, it is in fact this convergence (or one might paraphrase, 'the suppression of alternatives') which the Scollons (1979:11) take as problematic and in need of explanation, rather than the multiplicity of languages in use in the community.

Sankoff and Poplack's findings, in particular, raise more serious questions about the implication of social factors in linguistic change and the permissible extent of intereference between languages in contact situations. With regard to the first of these, for example, Dorian (forthcoming) argues that language contact and a highly differentiated social structure seem not to be of major significance in promoting change in East Sutherland Gaelic, although both these factors are often cited elsewhere. Thus, certain changes such as loss of tense distinctions cannot be attributed to contact with English. We have yet to formulate a theory of universals of language contact which will account for constraints on borrowing (cf. Moravicsik 1978). The cases I have cited here are illustrative of the difficulties involved.

Still, the fact remains that there do not appear to be any examples of communities in which vocabulary and pronunciation show less variation than syntax.[1] Unfortunately, sufficient data have not been forthcoming

which would allow us to assess the role standardization (and in particular, stylistic expansion and literacy) may be playing in this alleged process of supression of syntactic variation. I think there is good evidence to indicate that functional expansion and elaboration may condition or bring about the 'creation' of new syntactic categories or alternatives (cf. for example, Sankoff and Brown 1976 and Romaine 1980). Creoles and post-creole continua certainly provide numerous examples of this phenomenon. In fact, it seems that there is more variation in syntax in these situations than one might expect to find in a speech community which has arisen via a 'natural' process of dialect formation. This finding could however reflect a bias of interest on the part of creolists towards the investigation of syntactic differences as well as special history. Those working in urban speech communities have generally adopted the Labovian framework of quantifying phonological variables.[2]

At any rate, Hudson (1980:48) has put forward a tentative hypothesis about the different types of linguistic variables in relation to their imbrication with social structure. Syntax is the marker of cohesion; therefore, individuals try to eliminate alternatives in syntax. This does seem to fit the empirical findings of urban American sociolinguistics. Grammatical differences, e.g. multiple negation, have tended to show patterns of sharp stratification (cf. Wolfram and Fasold 1974) while phonological variables, like the raising of /ae/, reveal fine stratification. Thus syntactic differences seem to be more qualitative rather than quantitative. In contrast to syntax, however, vocabulary is a marker of divisions in a society (cf. for example, Bright and Ramanujan 1964); and we may find individuals actively cultivating alternatives in order to make more subtle social distinctions. And finally, pronunciation differences reflect the social group with which the speaker identifies. Individuals use some alternants and suppress others, thus using each in distinctive proportions relative to other alternants as a marker of group identity and solidarity. I think it is difficult to accept Hudson's view that syntax is a marker of cohesion when the syntactic differences which do exist in a community may be so divisive. One may also cite here the role of syntactic variation in contributing to so-called 'syntactic complexity' (cf. for example, Van den Broeck 1977 and Romaine 1980a and b).

This however concerns larger differences in the use of language rather than the use of isolated syntactic variables (cf. pps. 30-31).

These speculations are however best postponed or reconsidered more profitable when more data is to hand; and I am not concerned here with trying to account for the paucity of syntactic variation. The question I want to raise here is this: What kind of sociolinguistic theory does one need in order to talk about syntactic variation? Alternatively, we could ask, what is a syntactic variable?

Hudson (1980:157) dismisses the problem of how to define a linguistic variable because he says the notion of 'linguistic variable' is not intended to be taken as part of a general theory of language, but rather as an analytical tool for sociolinguistics. The distinction, it seems to me, is moot. Analytical devices are in a very real sense part of the theoretical assumptions one makes about the nature of the problem to hand, e.g. what it is like and what methods are relevant to its description. The assumption behind the extension of the linguistic variable from phonology to syntax is that syntax is in some way analogous to phonology; or more specifically, there is an analogous relationship between phonological and syntactic variation.

The differing semiotic functions of phonological as opposed to syntactic units are at issue here. The distinction between phonology and syntax depends upon the acknowledged properties of duality and arbitrariness. The success of Labovian methods in dealing with phonological variation can be attributed to the arbitrariness of the linguistic sign. A lexical item is an arbitrary pairing of sound and meaning (excluding of course, obvious cases of sound symbolism), which has to be learned as a whole. We could in principle change the phonological structure of every word form in a language without affecting at all the distribution of the resultant word forms throughout the sentences of the language or the meaning of the sentences. The relationship between the rules of syntax and the meaning of a sentence is by contrast iconic. We can't change the distribution of all the word forms in a language while holding the meaning of the lexemes constant or change the meaning of the lexemes without affecting the distribution of the associated word forms. In other words, we might say that sentences are not produced as unanalyzed holons,

but are interpreted anew on each occasion. We can compare a sentence with an algebraic equation (cf. for example, Vincent 1979), where the signs are not icons, but the structure of the equation is iconic. Thus, the symbols are arbitrary, but the relations among them are not. Similarly the words in a sentence are not icons, but the structure of the sentence is iconic.[3] As Lyons (1977:375) says, the theoretical conclusion to be drawn from this fact is that there is an intrinsic connection between the meanings of words and their distribution; and it is for this reason that it is difficult to draw a boundary between syntax and semantics.

It is this issue which is behind Lavandera's observation that the three examples presented by Sankoff (1973) to support the extension of quantitative methods of analysis to syntax were all cases in which the variation seemed not to be the carrier of social and stylistic significance. (To this, I would add that her three cases involved both grammatical and phonological constraints rather than purely grammatical or syntactic ones; but I will discuss the significance of this later.) According to Lavandera, the problem is that phonological variables which can be shown to have social and stylistic variation do not have referential meaning. I take her use of the term 'referential meaning' to indicate what others have called 'descriptive', 'cognitive' or 'conceptual' meaning (cf. for example, Lyons 1977). Nonphonological variables, however, may have social and stylistic significance (or what we may call 'stylistic' meaning) in a given case, but they always have cognitive meaning by definition. The difficulty is that the cognitive meaning must be assumed to be the same for all variants of the variable. The real dilemma then, is the difference in defining or assuming sameness of meaning for phonological as opposed to syntactic variants.[4]

Within this context it is easy to see why phonological variables were the safest starting point for quantitative analysis. According to Labov, social and stylistic variation presuppose the option of saying the same thing in different ways, i.e. the variants of the variables have the same referential meaning, but are somehow different with respect to their social and stylistic significance. The main contribution from quantitative studies which relied on the concept of the linguistic variable so defined was the demonstration that differences in phonetic realization

regarded as meaningless, i.e. cases of 'free variation', were in fact carriers of social and stylistic information in a systematic way. Labov's work thus put the study of stylistic meaning on a par with that of cognitive meaning.

Lavandera's main objection to the extension of the variable to syntax is that it reverses the whole significance of the concept as originally defined and used by Labov (1966). She cites in particular Labov and Weiner's (1977) study of the agentless passive, which depends on the assumption that the active and the passive can be used to say the same thing; or, in Labov and Weiner's words, the active and the passive, although different forms, are referentially identical.

It is not clear to me what is meant by the use of the term 'referential' by Labov and Weiner (or Lavandera, who has adopted their terminology in her argument). It seems to me that a lack of precision in defining terms like 'referential' is confusing the issue here. Labov has defined the variants of a variable as "alternative ways of saying the same thing from a truth-definitional point of view" (cf. Labov and Weiner 1977:6). This is of course only one possible way in which synonomy can be defined. Labov appears to accept in this case a kind of 'logical synonomy', i.e. two expressions are synonomous in a language if and only if they may be interchanged in each sentence without altering the truth value of that sentence. Logical theories differ of course as to how truth-value is arrived at, but I will ignore that here.

Labov however has gone on to conclude, in my opinion erroneously, that constancy of truth-value guarantees constancy of cognitive or descriptive meaning (what he imprecisely calls 'referential meaning'). This is not really true. We can say that two or more expressions are synonomous, i.e. have the same descriptive meaning or sense if they are substitutable for one another in a range of utterances without affecting their descriptive value. The point is that the implicational relationship between constancy of descriptive meaning and truth-value is not a bilateral one. Constancy of descriptive meaning implies constancy of truth-value; but the converse does not hold. We can substitute one expression for another which may result in an alternation of descriptive meaning, but not truth value (cf. Lyons 1977:202). If we take the two

sentences Unicorns exist. and Centaurs exist., we can say that these have the same truth-value (at least in this world, though they may well not in all possible worlds). In other words, both are false, but they have different descriptive meanings.

Labov's use of the term 'referential' is also problematic and misleading here since we are not dealing exclusively with referring expressions. I accept Lyons' (1977: chapter 7) view of reference as an utterance dependent notion, i.e. it applies to expressions in context and not to single word forms or lexemes. When Labov says that variants of a variable have the same meaning, what I think he should be saying (and meaning?) is that variant word forms don't change meaning. Here is where the trouble arises in talking about or defining syntactic and phonological variables in the same terms. If reference is a context and utterance bound concept, so that only expressions may have reference (lexemes have sense, and so do expressions), the study of syntactic variation must be concerned with the relationship between lexemes and expressions; the study of phonological variation, on the other hand, deals with the relationship between lexemes and forms. The sense of an expression can be thought of as a function of the senses of its component lexemes and of their occurrence in a particular grammatical or syntactic structure. The only reference an utterance can be said to have is to its truth-value.

In their study of the passive Labov and Weiner (1977) claimed that the choice between the agentless passive and the active under certain conditions was constrained entirely by syntactic factors, and carried neither social nor stylistic significance. As Lavandera quite correctly points out, Labov and Weiner are really arguing that variation between the active and passive is meaningless in terms of three dimensions, referential (in the sense of 'cognitive' or 'descriptive'), social and stylistic. This amounts to saying that the observed frequencies with which the active vs. passive occur relates only to surface structure constraints and does not convey any information. It is this aspect of the difference between phonological and syntactic variables that bothers Lavandera.

The kind of meaningfulness which Labov's study of phonological variables was originally concerned with relates to actual utterances in specific contexts. In other words, sameness and difference in meaning is assumed at a surface level of utterance in the case of phonological vari-

ables. In the case of syntactic variation however, what is being assumed is equivalence of underlying syntactic structures. Lavandera takes the argument in a somewhat different direction from here, which I will not pursue because it is tangential to the issue at hand.[5] She seems to have become confused by, or overly concerned with Labov's contradictory use of the linguistic variable, and hence misses what I consider to be the more important issue. Moreover, Labov's (1978) reply to Lavandera on this question of where the sociolinguistic variable stops does not really speak specifically to or shed light on the point she is making. It is merely a reiteration of methodological principles and Labov's position on the relevance of the study of variation to linguistic theory. He says, for example (1978:5):

> She [i.e. Lavandera SR] is right in not being persuaded by our arguments [i.e. that they broke into the liquor closet means the same thing as the liquor closet was broken into. SR.] But we are not in the business of being persuasive; our enterprise demands conclusive demonstration.

The problem which arises in describing precisely the relationship between lexemes and expressions on the one hand (i.e. syntactic variation) and between forms and lexemes, on the other (i.e. phonological variation) is that this cannot be done except within the framework of some specific grammatical theory. Furthermore, these relationships might have to be stated somewhat differently for different languages (cf. for example, Lyons 1977:25). The Labovian formulation of the concept of linguistic variable adheres very closely to an early Chomskyan conception of generative grammar (cf. for example, Labov 1969:761). In its classical (or standard) version, i.e. Chomsky (1965), transformational generative grammar is well suited to the analysis of certain kinds of variation. It allows us to say, for example, that superficially identical phrases and sentences may be derived from distinct underlying structures, and conversely, that superficially distinct sentences may be derived from the same underlying structures. Labov would have had much more difficulty with a non-transformational generative grammar, e.g. categorical grammar.

The concept of underlying structure allows Labov to say that there is some syntactically distinct and identifiable unit which is constant

over its variant realizations and that it is this unit of expression which has the semantic function of reference. In the case of the active and the passive for instance, a transformational grammar allows us to derive one of these from the other (or both from an identical pre-sentential structure of a more abstract kind). The original Katz and Postal (1964) argument held that transformations had no effect on the meaning of sentences; Labov and others have since taken an altered and more defensible view that the truth-value of sentences is held constant under transformations. This position avoids the unattractive conclusion that there is no distinction between the meaning of a sentence and the meaning of an utterance. Thus, stylistic variation no longer has to be classified as semantically irrelevant.

The classical view of generative phonology has similarly been put to good use by Labov in the study of phonological variation. The Chomsky and Halle (1968) model of phonology presented in The Sound Pattern of English assumes that speakers of all dialects of English, for example, share the same underlying phonological representations, which are relatively stable over time. Thus, change (and hence also variation) occurs in the rule component and not in underlying forms.[6] The Labovian conception of the speech community is strongly tied to generative transformational views about how grammars are organized. Labov assumes that everyone in a particular speech community has the same underlying phonological and syntactic representations, so that variations in pronunciation and syntax have their locus in the rule component of the grammar, more specifically in the form of so-called 'variable rules' (cf. Labov 1969). This formal machinery allows one to maintain an underlying unity among speakers of a speech community, which may exist or make sense only from a descriptive point of view (cf. also Romaine 1980c and 1979).

It is at this point that one must raise the question of what kind of syntactic and sociololinguistic theory one needs in order to talk about syntactic variation coherently. Labov is approaching the problem of defining sameness of meaning from a very limited perspective. In other words, his model allows us to deal with alternative constructions/sentences which can be used with the same truth-value in identical contexts. However, the notion of assigning truth-values to propositions expressed

by sentences is satisfactory for only a relatively small subset of sentences in English (and probably any language). This in effect limits the study of syntactic variation within a Labovian quantitative framework to cases which are, in my opinion, relatively trivial, and largely uninteresting.

Let's take a seemingly simple and straightforward illustration of what one would like to call 'syntactic variation'. If we look at the following four sentences, we can, I think, see that they are in some sense related or are variants of one another.[7]

1. Because it was cold, I closed the door.
2. I closed the door, because it was cold.
3. It was cold, therefore I closed the door.
4. *I closed the door, therefore it was cold.

It is relatively easy to account for (1) and (2) as variants of the same syntactic variable. All that varies is the order of occurrence of lexemes in a particular grammatical structure. The important thing is that we must be able to maintain that the variation in the order of constituents does not alter the truth-value of the two sentences, i.e. the sense of the lexemes must remain the same regardless of their order. The question for empirical research then is to investigate what external or social constraints, if any, condition the variation. I have not done this, but I suspect that it would prove uninteresting (as in the case of the active vs. agentless passive), at least if one confines the search for conditioning factors to the usual Labovian ones of sex, age, social class, etc. I think the relevant external factor in this case is what one might call a pragmatic constraint of foregrounding vs. backgrounding; but this is generally not the kind of extralinguistic factor Labov is concerned with when he speaks of the 'social conditioning of linguistic structure'. Thus even a relatively trivial instance of word order variation can cause difficulties.

Things become even more troublesome if we try to 'extend' the variable to include the third sentence (assuming we allow for the fact that the variable may be socially meaningless in the usual Labovian sense). One must now ask whether for example, (2) and (3) and (1) and (3) mean the same thing. Sentences (2) and (3) and (1) and (3) contain the same basic

propositions, but differ in one lexeme (i.e. the connective therefore vs. because), and order of constituents. Intuitively, one would like to say that these are variants of the same variable, but in a Labovian framework one must rely on truth-value. In other words, one must ask whether the lexemes concerned and their relation (defined syntactically) in these sentences affect their truth-conditions. Generally, logical connectives such as therefore and conjunctions such as but etc. are not regarded as contributing to the sentence meaning in a way which alters truth-conditions.[8]

The kind of variation we are dealing with in these first three instances might be better refered to as 'textual'. I am using the term 'textual' here in the sense given to it by Halliday and Hasan (1976). According to their view the textual component is one functional semantic component of the linguistic system. The textual component has to do with the resources a language has for creating cohesive discourse. Connectives such as therefore and because are included here; these are elements which are subject to referential verification but which can only be understood in terms of pragmatic discourse function. There are two other components in the system, the ideational which is the main locus of truth-functional relations, and the interpersonal, which has to do with speaker attitude. Thus, we could say about these sentences that variation lies in the textual and possibly also interpersonal component, rather than in the ideational. In the first sentence because is used cataphorically to connect the two propositions, while in the second it is used to mark the antecedent relation to the first proposition, but is still cataphoric to the following subordinate proposition. In the third sentence therefore marks the consequent relation in a subordinate clause, but is anaphoric to a preceeding proposition. We can say that we are dealing with variation in cataphoric and anaphoric markers of consequent and antecedent propositions; but this is not the Labovian definition of syntactic variable. Even more illusive is how one explains the ungrammaticality or non-occurrence of the fourth potential variant. In Old English it was possible to construct a sentence which parallels the second one in terms of order of propositions and which connects the two propositions by means of a logical connective indexing the reason. Because such a construction type is no longer possible in English one must account for the process by which new forms and meanings arise and die out in a language.

Traugott (1979) has argued that data on the development of markers of antecedent and consequent relationships demonstrate that the resources for marking textual relationships actually increased between Old English and Middle English, i.e. there is now a larger number of logical connectives. This example suggests the importance of looking at pragmatic meaning from an historical perspective; but I will ignore the diachronic implications of this example until I have sketched out the kind of theory within which one can talk about syntactic variation.

Hudson (1980:189-90) has also suggested that transformational generative grammar is an unpromising theory for dealing with the relationship between social context and linguistic variables. The problem according to Hudson is the fundamental distinction made in transformational generative grammar between patterns which are defined indirectly by means of phrase structure and transformational rules. He prefers a model of language structure in which there is much less difference between lexical items and syntactic structures into which they fit.

He specifically suggests using the term 'linguistic item' instead of linguistic variable, which will cover not only variation in phonology, but also variation in morphology and syntactic structure. According to Hudson's definition, a "linguistic item is simply a pattern which may be identified, at any level of abstraction in the structure of a sentence". The justification for the treatment of different kinds of items within a more general approach is that the facts of social distribution are of much the same type, whether the item concerned is a lexical item (to use Hudson's example, pussy in contrast with cat) or a construction (e.g. Teddy fall down in contrast with Let Teddy fall down), or a phonological pattern (e.g. /t/ instead of /?/), or a morphological one (e.g. goed instead of went).

I am very much in sympathy with Hudson's reasons for wanting a more coherent theory than transformational grammar within which to embed such instances of variation in a systematic way. He claims that the more these patterns are treated differently in a grammar, the harder it is to explain why they are all related to social context in the same kinds of ways. I wouldn't agree with his latter claim. First of all, not all linguistic items are related to social context; and I think sociolinguistic theory has this fact to 'explain'. Secondly, and more importantly however,

Linguistic items are not all related to social context in the same kinds of ways, as I hope to show. Again, the problem boils down to this: How does one define syntactic variation and what conclusions can be drawn from the cases which have been studied so far?

Typology of linguistic variables

If we take a typological approach to the kinds of variables which have been discussed in the sociolinguistic literature so far, we can see that there is support for the argument that syntactic variation intersects with social and stylistic factors, if indeed at all, in a different way than other linguistic variables. (I will consider later whether this is an artefact of the theoretical framework in which they are defined, as Hudson claims, in the next section).[9] In the following diagram I have grouped some of the types of linguistic variables studied so far in terms of their conditioning factors.

Typology of variables in terms of linguistic and social conditioning factors

Type of variable	Conditioning factors			Example
	linguistic	social/stylistic		
1. 'pure' phonological	phonological	yes	yes	postvocalic /r/
2. morphophonemic	phonological/ grammatical	yes	yes	t/d deletion
3. morpho-syntactic or morpho-lexical	phonological/ grammatical	yes	yes	complementizer *que* deletion
4. 'pure' syntactic	syntactic (?)	no (?)	no (?)	agentless passive

The basis for my typology lies in both the relevant conditioning factors which are operative as well as the way in which the variable and its variants are defined. Phonological variables typically make reference to purely phonological conditioning environments.[10] An example of this type would be postvocalic /r/ in New York City, whose realization is dependent upon the nature of the following phonetic context, i.e. whether a word beginning with a consonant or a vowel follows. Other so-called 'phonological variables' require statement of both phonological and grammatical conditioning factors. This type I have called morphophonemic.

A relevant example is t/d deletion, which is governed by a first-order phonological constraint (i.e. whether a word beginning with a consonant or a vowel follows), and a second order constraint, which has to do with the morphological complexity of the word form (i.e. whether it is monomorphemic or bimorphemic).

In this case and others like it I think one might reasonably argue that we are dealing with two variables instead of one. That is, in one case we have monomorphemic forms like mist, and in the other, bimorphemic forms like missed. Thus we could separate the variable into two phonological variables. If we did this, we might in fact have an explanation for the bimodal distribution found in connection with this variable, which seems to have particularly bothered Guy (1974 and 1977) and others.[11] Thelander (forthcoming) has also recently suggested that after dividing all instances of a variable into linguistically defined subcategories, only those which exhibit similar patterns of social variation should be aggregated (cf. also Romaine 1979a). Whether one wants to make a distinction between morphophonemics and phonology or between morphology and syntax will depend of course on the language concerned; but this is an issue I won't deal with here (cf. also Linell 1979 for arguments in favor of keeping morphophonemics separate from phonology).

The third type, which I have termed 'morpho-syntactic or morpho-lexical', has also been referred to by others as grammatical or syntactic variation. I have cited complementizer *que* deletion as an example. Others might include negative concord (cf. Labov 1972a), negation in Montreal French (cf. Sankoff and Vincent 1977), and complementizer *that* deletion (cf. Kroch and Small 1978). Here the conditioning factors may be both phonological and grammatical. I think a distinction needs to be drawn between this kind of variable and what I will call a 'pure' syntactic variable like the agentless passive.

The basis for the distinction again lies in both the relevant conditioning factors which are operative as well as the way in which the variable and its variants are defined. We may note firstly that the third type (as well as the first and second) is affected by both linguistic and social factors, whereas the pure syntactic type is not. (Or, at any rate I do not know of a clear instance which is, and which also meets the criteria I will set up here).[12] Secondly, in the case of morpho-syntactic

variation we are dealing with the presence or absence of some linguistic item. However, the notion of some syntactic variation suggests to me that a whole construction or arrangement of items which alternates is required. The issue which arises in connection with syntactic variation defined in these terms is whether there are cases of complementary distribution within some syntactic construction which are analogous to phonological and morphophonemic alternations based on complementary distribution within a paradigm.

For example, Sankoff and Thibault (1977) have argued that the alternative auxiliaries avoir and être in the French periphrastic perfects are in complementary distribution with respect to the set of verbs in the language. This in turn allows us to establish some more abstract grammatical pattern, e.g. aux + stem + past participle ending.[13] Even this example, however, does not require a drastic alteration in the Labovian concept of linguistic variable because Sankoff and Thibault deal only with presence vs. absence of the variants in question. They (1977:105) find that three sub-groups of speakers can be isolated in terms of their use of avoir with intransitive verbs:

1. those who systematically used avoir (N = 4/119)
2. " " " être (N = 11/119)
3. " " variably " both (N = 104/119)

Generally speaking, the variable is sensitive to a social constraint to the effect that the higher a person's social position, the less he uses avoir. The question which arises is whether there is any semantic distinction involved in the present social variation, or whether it is merely the residue of a defunct aspectual system. Again I will postpone discussing the diachronic implications in the process of grammaticalization. This variation has been noted by grammarians for the past three centuries; some said that être was used to indicate perfective aspect. Upon closer examination of the verbs which co-occurred with avoir/être, they found that the first group of speakers was using an aspectual distinction; namely, avoir was reserved for use with verbs of movement which were [-complete], but être occurred with the same verbs if they were [-complete]. The second group appeared to express the aspectual distinction because they systematically used être. The third group, on

the other hand, didn't observe aspectual distinctions in the auxiliaries except for a few verbs.

This is really as far as their analysis goes; as it stands, the case of avoir/être doesn't really constitute an example of syntactic variation as I would like to propose that it be defined because Sankoff and Thibault have completely ignored the problem of how the second and third groups express the aspectual distinction drawn by the first group, if indeed they do. This is the problem posed by syntactic variation.[14] We can see that it involves us squarely in a long-standing controversy over the borderline between syntax and semantics on the one hand, and between semantics on the other, not to mention the relationship between sociolinguistics. But perhaps the most crucial issue concerned is how one accounts for meaning and function with a sociolinguistic theory.

Meaning and function within a sociolinguistic theory

I think there are serious limitations in the notion of syntactic (or for that matter, semantic) variable within a Labovian (or indeed any traditional) grammatical framework. We can't hope to gain much insight into the function and use of utterances if we start by assuming that the referential function is basic and by looking at variation against the backdrop of referential constancy. Labov's approach starts with a basically linguistic semantic or referential analysis from which the linguistic analysis and arrangement of items in syntactic structures emerges in the traditional way. Then he adds an account of how these correlate with certain extralinguistic categories, e.g. age, sex, social class, etc. Labov assumes that sociolinguistic choice is dependent on the recognition of representational sameness. Thus, in his view, sociolinguistics presumes (or is parasitic on) the existence of a mode of linguistic analysis (or a linguistic theory) which has established a framework for identifying and dealing with alternate ways of saying the same thing in terms of logical form. However, the relevant analytical categories which emerge from an analysis of speech in terms of purely referential and internal linguistic categories are not necessarily going to be the same as those which emerge from other functional modes, such as those suggested by Halliday and Hasan (1976).

Silverstein (1976) has veen very critical of this kind of approach, and in particular, of Searle's speech act semantics because it tacks onto traditional grammatical analysis a description of how these referential categories can be 'used' performatively. This method holds water only if tokens preserve their reference in all the speech events one observes, as I suggested earlier. A functional analysis, on the other hand, presupposes that we can define isofunctionality of utterances while holding other modes of meaning constant. I would maintain that one central insight of speech act semantics is relevant to the problem of syntactic variation. Namely, we use language to do things and describing is only one of the things we do with language. Thus, we can say that the same communicative intent (or function, if you like) can be realized by linguistic means (i.e. surface syntactic forms) which are so different that the variants exhibit virtually no phonological, lexical or semantic similarity. For example, the following sentences may be thought of as functionally equivalent in that they have the same communicative intent:

1. It's cold in here.
2. I'm cold.
3. Are you cold?
4. Would you close the window?
5. Close the window!

That is, one may say all of these to a person to get him to close the window (cf. also Ervin-Tripp 1976:125). These are all ways of 'saying the same thing', loosely speaking, which do not have the same referential meaning and cannot be defined exclusively in terms of constancy of truth-value. They may be thought of as different ways of using language to achieve the same communicative intent; or, in other words, what we are saying is simply that one particular communicative intent may be realized by a variety of linguistic structures. Conversely, one particular arrangement of linguistic items in a surface syntactic structure may have different communicative intents and uses. For example, I'm cold may well be a command in some cases as well as an apparent statement of fact in others.

For those who hold the view that the semantics of natural language must be exclusively or largely truth-conditional, the notion of defining meaning in terms of communicative intent constitutes a radical change in our conception of truth. In other words, it can no longer be defined

as a relation between an abstract proposition (statement or sentence) and a particular state of affairs, but only in relation to the speaker, hearer and context. Sentence and word meaning are thought of in terms of the conditions which must be met for their appropriate use. Implications of sentences will then be characterized not as the property of the sentence itself, but as a presupposition on the part of the speaker using that sentence (i.e. felicity conditions), or in other words, a requisite set of beliefs if the speaker is to use the sentence appropriately to achieve a particular communicative intent. So the assignment of truth-value is dependent on what information the speaker is intending to convey to the hearer.

If we return to Labov and Weiner's (1977) case of variation between the active and agentless passive, what this amounts to is that the passive form of a sentence may be false, while under the same circumstances the active form may have no truth-value at all. In addition, the same active sentence may be false in one speech act situation and truth-value-less in another, while the state of affairs to which the sentence refers remains constant. Thus, Labov and Weiner's sentence, The liquor closet was broken into, where the liquor closet is the topic, will lack a truth-value if there is no liquor closet.

Kempson (1977:60) and a number of others have pointed out some of the problems which result from the failure to maintain a boundary between semantics and pragmatics. If we derive the interpretation of sentences only from speaker's knowledge and beliefs (i.e. in terms of presupposition and conversational implicature etc.), then the meaning of a sentence cannot be determined independent of the speaker of a sentence in a particular speech act situation, and sentences then become indefinitely ambiguous. Kempson maintains that without definable limits on semantic theory, semantics becomes an inoperable discipline. We would be faced with an analysis of meaning which claimed that every sentence had an indeterminate number of representations and sentences would not have a particular meaning apart from the context in which they are spoken. A theory which is based on this kind of speaker relative concept of meaning, or what she calls a performance (rather than a competence) theory, isn't able to account for either the meaning of sentences or the meaning relationships among senten-

ces (e.g. entailment, synonomy etc.). Kempson's (1977:55) conclusion is that there is no conflict in principle between a truth-conditional account of semantics as a part of a theory of linguistic competence and a speech act theory of utterance as part of a theory of performance or communicative competence, as long as the two are kept separate.

I would like to argue that this is not a necessary conclusion and that there are dangers in accepting this dichotomy which are at least equally serious as those entailed in the acceptance of Kempson's argument that semantics in order to be operable must remain autonomous and logically based. I agree with Silverstein (1976:18), for example, that it is presumptious to speak of arrangements of a basically propositional (i.e. referential) nature being 'used' in other ways. Reference is but one of the many functions of utterances and not the basis for all others. He believes that the bias towards pure referential categories is one of the principal reasons why social functions of speech have not been built into our analyses of language; this has long been the case, in spite of the fact that most of what goes on in any speech event is not purely referential.

What I am suggesting here is of course by no means novel. Firth (1966), for example, treated the descriptive function of language as something that was subsidiary to and part of the more important and general social (i.e. indexical) function of language. He never made it clear, however, how non-indexical reference could be handled by means of function in context. And, more recently Halliday (197_) has based his work heavily on the belief that one of the most distinctive aspects of adult human speech behavior is the multifunctionality of utterances. In his studies of child acquisition he has suggested that children do not initially use utterances multifunctionally, i.e. a given utterance tends to have only one interpersonal function.

One might reasonably conclude that what I am arguing for (like, for example, Dines 1980) is an extension of variation analysis to the level of discourse, or even beyond syntax into semantics and pragmatics. It is of course arguable whether syntactic variation is distinct from semantic variation. If every generalization we want to make about the syntactic structure of the sentences of a language has 'explanations' in terms of the meaning of those sentences, then syntactic generalizations

are actually ipso facto semantic ones. Similar comments apply to the distinction between semantic and pragmatic variation (cf. also Kempson 1977:60). 'Extending' variation analysis to these levels beyond phonology involves refining the notion that variants can be defined in terms of referential equivalence. What Lavandera (and Dines) are questioning is whether this extension requires a modification in the nature of the underlying form of the linguistic variable, i.e. the defining criteria of the variable. I think it requires much more, namely, a modification in our view of the nature and goals of a sociolinguistic theory and the place of such a theory vis-à-vis linguistic theory.

Lavandera suggests rather tentatively that functionalism is important; and Dines specifically proposes that variables may be postualated on the basis of common discourse function. This notion of functional equivalence is crucial, i.e. variants must be seen to be related in terms of common function rather than in terms of conventional phonological, syntactic or semantic forms, or logical equivalence. The latter is of course the classificatory basis used by Labov. Now I am not implying by any means that Labovian methods are fruitless, but merely that Labov seems to have retreated from an earlier position in which he claims to have been interested in pursuing the role of social factors (even though narrowly construed, I would claim) in language change and differentiation. That is, Labov seems to have had no doubt that social conditioning factors were operative in linguistic processes, the question was how deeply certain kinds of linguistic rules were affected by social constraints (cf. for example, Labov 1972b:226). Now, however, in contrast to his earlier work on Martha's Vineyard and in Harlem with Black English Vernacular, Labov seems to be more pre-occupied with the analysis of the linguistic (rather than social) constraints underlying variation. There is a decided emphasis in recent North American sociolinguistics on the development of more sophisticated versions of probablistic variable rule analysis and other analysis and other analytical techniques for studying the internal organization of linguistic variation. But this advance in analytical precision has unfortunately often been at the expense of a coherent model for the analysis of language use and social meaning, and in particular, pragmatic resources for meaning.

In principle, there is no conflict between a contextual and a truth-

conditional theory of meaning, as Kempson suggests; but I think it is at least arguable that what is needed is a more comprehensive and integrated theory which subsumes both approaches and shows how they are inter-related. In order to be truly comprehensive, a theory of language would have to explain not only what is expressed and how (i.e. the set of possible utterances in language and the relations among them), but also who says the same thing and when (i.e. the distribution of utterances over social groups and contexts). If one accepts the view that one goal of a socially-constituted theory of language is to produce an account of communicative competence, then it is just as reasonable to say that someone does not know the meaning of a word/expression if he cannot contextualize it as it is to say that he doesn't know the meaning if he doesn't know its truth-conditions.

The problem with keeping a theory of language use projectionist, i.e. separate from an autonomous linguistic theory which deals with decontextualized or depragmatized system sentences, is that social context and meaning is relegated a place of secondary importance. That is, context is to be invoked just in case a truth-conditional account of the meaning of sentence fails or is inapplicable (cf. Lyons 1977:611-2). This raises the question of what place sociolinguistics should occupy vis-à-vis linguistics.

Labov's approach is based on the belief that the validity of sociolinguistic research is to be measured in terms of its ability to relate sociolinguistic data to the central problems of linguistic theory (cf. for example, Labov 1972a:183-4). What I am arguing is that this is surely too narrow a conception of the role of sociolinguistic research. The social and linguistic differentiation which forms the basis for Labov's 'sociolinguistic patterns' remains uninterpretable and largely static correlations between certain linguistic phenomena and social categories unless these data are seen in terms of a more general sociolinguistic theory of the kind suggested, for example, by Hymes (1974a) and Le Page (1978). If such a theory attempts to make a coherent statement about the relationship between language use and social structures of various kinds, I would maintain it cannot be founded on the narrow base Labov envisions.

If what I am arguing is tenable, then it should warn us against the belief that <u>extending</u> traditional linguistic analysis, in particular its

basis in a theory of meaning founded on the sameness/difference of utterances in terms of referential propositions, and retaining the same methods and assumptions about linguistic categories, will be adequate to the task of investigating variation at levels above the phonological. In order to gain insight into the study of language use and function, it will not do to patch up traditional (in particular, transformational generative) grammar.

Now it is clear that some alteration to our ideas of what a grammar is will be required (cf. also Hymes 1974b). Silverstein (1976:20) for example, has suggested that we call the study of the meanings of linguistic signs relative to their communicative functions pragmatics. Thus, these meanings are 'pragmatic meanings'. For Silverstein then (as for Firth), semantic or referential meaning is a special form of pragmatic meaning, i.e. it is simply that mode of signification of signs that contributes to purely referential function. There is however some difficulty, in my opinion, in assuming that one set of categories, i.e. either referential or pragmatic, is basic. But in particular, there is a very serious danger in basing linguistic analysis on purely logical or referential categories. That is, we have not seriously or sufficiently investigated the possibility that all languages may not have the same underlying logico-semantic structure (cf. for example, the discussion in Traugott 1979:18). The very posing of this question has been hindered by the belief in the equality of all languages. While the view that no meaning can exist without a surface form to express it is false, it is by no means certain that the opposite view is false, i.e. that semantic relations and categories such as definiteness exist, whether or not they are explicitly marked. Labov (1970), for example has questioned whether there is any semantic difference between a language without tense markers (i.e. one which marks time relations with temporal adverbs) and one with tense markers. He believes the distinction is stylistic only. We have no way of knowing these things yet.

We may bring in the issue of syntactic complexity here as another case in point. Schulz (1972:101), for example, has concluded that in colloquial German the particles ja, doch, da etc. (so-called Füllwörter), are just as capable of serving as logical connectives as the causal conjunctions weil, daher, etc. of the standard language, which require

subordinate clauses with transposed word order. The use of the latter has been cited as one particularly salient and defining characteristic of the elaborated code. Compare, for example, the following sentences (Schulz 1972:101):

1. Meistens, nachmittags, geh ich dann mit die Kinder raus, die müssen ja auch frische Luft haben.
2. Meistens gehe ich nachmittags mit den Kinern draussen, weil sie frische Luft haben müssen.

What Schulz is saying is that from a functional point of view, the particles and conjunctions are equal or are in other words to be understood as functionally equivalent variables in the expression of discourse cohesion. This particular example in which the equation is between paratactic constructions with Füllwörter and hypotactic constructions with causal subordination conjunctions and transposed word order is similar to the English one I cited earlier (cf. p. 13). In both cases we are dealing with differences in lexemes and the order of constituents in propositions. But is the difference only functional? I.e. if a language/variety has only one marker which can be used in discourse to connect propositions in a causal relationship, is the semantic relationship still there? In other words, as Traugott (1979:18) has queried, is a relatively paratactic stage of a language only syntactically and pragmatically different from a more hypotactic stage, or is it also different semantically? Traugott argues that unless we allow for change/variation in semantic relationships, then we are forced into the view that whenever language came into being, it had its full set of semantic relationships inbuilt.

Some possible frameworks for understanding syntactic variation

There are a number of approaches to understanding grammatical structure which provide a framework within which syntactic (and indeed other kinds of) variation can be accounted for. I can think of at least three; namely, the form of grammatical structure can be seen in terms of the relationship between:

1. the logical structure of utterances (i.e. truth-value) and surface syntactic form (cf. for example, Keenan 1975 and Bartsch and Venneman 1972);

2. perceptual processing and syntactic process (cf. for example Bever and Langendoen 1972 and Lightfoot 1979); and
3. conversational organization/interaction and syntactic structure.

Labov 'explains' syntactic variation in terms of the first kind of relationship. The other two approaches are concerned with locating sources outside the purely linguistic system that might motivate the bulk of grammatical constraints. The second of these possible loci, i.e. cognitive processing and perceptual strategies, we might call an 'internal' functionalist and the third an 'external' functionalist perspective (cf. Brown and Levinson 1978:261). The internal approach is receiving more attention recently in the study of pidginization/creolization, where it is particularly evident that the creation of certain semantic categories and syntactic structures suggests common cognitive strategies at work. Recent work in pragmatics and in particular, the analysis of conversation and discourse has been concerned with linking linguistic structure to the organization of communication. One important hypothesis which this approach would want to explore is this: To what extent can the set of natural syntactic processes found in language be regarded as due to constraints which stem form the organization of conversation? This is just another way of asking whether there are cases where pragmatic considerations condition syntactic form and where pragmatic distinctions are grammaticalized in natural languages (cf. for example, Creider, forthcoming on thematization in Luo in Sag and Hankamer 1977 on pragmatically controlled anaphora).

It would not be difficult to argue on the basis of not very exiguous data from languages such as English (in which the bulk of syntactic and semantic theorizing is based), where there is little formal marking of thematic structure, that an autonomous semantics (which is truth-conditional), is logically prior to and separate from a pragmatic theory which deals with use. But this view is much less tenable in a language like Luo, where pragmatic distinctions apparently have been grammaticalized. It would be difficult to argue that pragmatics was totally independent of syntax or that syntax and pragmatics were autonomous. Similarly, Brown and Levinson(1978:284) suggest that the difficulties posed for a sociologically sensitive grammatical description of Japanese seem insuperable.

Japanese syntax and semantics are profoundly affected by the impingement of social forces on the pronominal and verbal systems.

If we follow through with an approach to language which is equally responsive to both internal structural and external social pressures on grammar, then a pragmatic account of meaning, i.e. meaning relative to function, should form an important part of an integrative sociolinguistic theory. If this means that what sociolinguistics is about is the differential use of pragmatic resources by different speakers in different situations, then sociolinguistics in its broadest possible conception is, as Brown and Levinson (1978:286) maintain, 'applied pragmatics'. It should be stressed however, that this is not just 'tacking' discourse onto syntax and semantics.

The question we really need to ask ourselves at this stage is whether we are willing to accept a theory which cannot handle all the forms/uses in which variation may manifest itself in a given community over time and which cannot provide a coherent account of how these particular functions, uses and kinds of variation develop within particular languages, speech communities, social groups, networks and individuals. I would like to conclude by sketching out very broadly and generally what one can do with a theory which recognizes the importance of the third perspective I have mentioned and which also sees grammar in terms of the imbrication of the ideational, textual and interpersonal components of the semantic system discussed by Halliday and Hasan (1976). I will try to illustrate how such a view allows us to see not only diachronic and synchronic variation, but also variation in acquisitional and post-creole continua within the same framework.

If we return to the examples of textual variation in logical connectives discussed earlier (p. 13), it is not hard to argue from a diachronic viewpoint that the kind of theory which will account for the development of certain logical connectives, e.g. <u>while</u>, <u>hence</u> etc. used to express cohesion between utterances is one based on the pragmatics of discourse rather than logic. Traugott (1977) has documented the historical origin of these in spatial or temporal elements. The grammaticalization of the latter was accompanied by a shift from concrete to abstract meaning. In other words, the progression was from temporal ordering of concrete objects in a sequence or ordering of events along

a time line to ordering of propositions in discourse.

To take one specific example, the derivation of the German connective folglich from A earlier than B to A therefore B is a good illustration of the tendency to infer cause from temporal sequencing. While this kind of development might also have a seemingly good explanation in terms of human perceptual mechanisms, it is probably more profitably regarded as motivated by speaker organization of discourse (cf. Traugott 1977:15). La Brum (1979) has also recently suggested there is evidence that whereas and thereas developed their present contrastive connective meaning through use in certain discourse contexts.

Sankoff and Brown (1976) have suggested a similar strategy is at work in Tok Pisin in the creation of the relativizer ia and that one must look at the origins of syntax in discourse. They propose the following development:

1. ia is a place adverb (from English here);
2. extension as a postposed deictic or demonstrative (e.g. dispela man ia- 'this man here');
3. further extension for general bracketing, including topic-comment structures, relativization and cleft sentences.

Creoles of course provide excellent opportunities for the observation of 'grammaticalization in progress'.

It is not difficult to see why Traugott (1977:13) has argued that the kind of theory of grammar within which certain semantic-syntactic changes (and one could also say, variation) need to be embedded is one which includes the pragmatics of discourse. Logic, she suggests, is the end rather than the starting point of the derivation. She is referring here specifically to the development of logical connectives from space-time adverbials in English. While this is certainly true for some phenomena within diachronic and post-creole continua, one would also want to explore the ways in which new forms and meanings arise and are lost in other continua (e.g. child and second language acquisition and language death) via shifts (not always unidirectional) from one component of the semantic system to another. Thus, for example, Halliday (1975) suggests that the 'starting point' for children acquiring their native language is the interpersonal; it would be interesting to look at second language acquisition to find out how the advancement of the semantic system takes place.

Diachronic developments which involve meaning shifts from the ideational (or propositional) to textual to interpersonal are well-evidenced (cf. Traugott 1979). The problem lies not in deciding which set of categories or which semantic component to take as basic (i.e. as the absolute starting point, and trying to account for another by means of reference to more fundamental or underlying ones), but rather in mapping of the relations between them, i.e. the various routes between one and the others which are possible.

The intersection of these variation continua, i.e. diachronic, synchronic, acquisitional, etc. shouldn't of course be surprising, given what we know about the relationship between variation and change; but the parallels among them become clear only within a functionalist perspective, i.e. when one considers how linguistic means are shaped by their place within and in terms of their relation to the communicative patterns of human societies.

FOOTNOTES

1. The mother-in-law language described by Dixon (1971) in Dyirbal is a nice illustration of this. There is an everyday set of lexical items and a 'mother-in-law' set which must be used by a speaker only in the presence of his mother-in-law. Grammatical structure remains exactly the same in these two situations. What changes is the entire set of lexical items.
2. I have discussed the relevance of this in relation to arguments between proponents of the quantitative vs. dynamic paradigms in Romaine (forthcoming).
3. Vincent (1979) has discussed a similar gap between phonology and syntax in dealing with the problem of syntactic reconstruction. The feasibility of phonological reconstruction rests primarily on two assumptions: 1) the arbitrariness of the linguistic sign; and 2) the regularity of sound change. The techniques of phonological reconstruction do not bring the two halves of a linguistic sign any closer together; this is however not so in syntax.
4. There are of course other problems which I will not deal with here. Lavandera, for example, does not comment on the consequences of the conception of the linguistic variable as a continuous dimension of variation. Syntactic variation produces a finite number of discrete variants so there is no surface continuum of realizations to be dealt with (cf. also Naro 1978). However, this concept of variation organized along a continuum has proved difficult to apply even to certain types of phonological variation (cf. Romaine 1979).
5. Lavandera observes that one of the reasons for restricting the study of sociolinguistic variation to cognitively meaningless surface variants is out of fear of providing a linguistically based argument for cognitive deficit theories (cf. also the discussion here on syntactic complexity, pp. 24-25). One may always still hold the conclusion that there are different ways of communicating the same cognitive meanings without assuming that there are differences in ability to do so. In other words, we might say that within a community different groups have different 'styles' (in Hymes' 1974b sense). This avoids the problem of 'differential competence'. This is also Van den Broeck's (1977) and

Schulz' (1972) interpretation of the significance of syntactic complexity.

6. Some of the early transformational accounts of syntactic change also assumed this (cf. for example, Traugott 1972).

7. I am grateful to Elizabeth Traugott for suggesting these examples and discussing with me some of the issues raised by them. She however is not responsible for the use I have made of them here.

8. A statement of the meaning of therefore in a sentence P and therefore Q would have to give the set of conditions guaranteeing the truth of P and Q together with a condition specifying that Q follows from P. If this condition is not truth-conditional, it should not affect the truth-value of the two sentences it conjoins (cf. also Kempson 1975:207; 213-8).

9. Work on syntactic complexity suggests that syntactic differences intersect with social and sytlistic continua in a way opposite to phonological ones. Van den Broeck (1977) has argued that the latter are more evident in informal styles, while the former are in more formal styles.

10. In certain complex linguistic situations we might want to recognize lexical conditioning of certain variables. For example, the Scots form of the negative (i.e. -nae) can be attached only to a certain class of auxiliary verbs and certain phonological alternations take place only in restricted word classes (cf. Romaine 1975). But these cases might better be regarded as a problem in determining word class membership.

11. This might also clear up the somewhat puzzling reversal of social constraints in the case of Wolfram and Christian's (1976) study of a-prefixing in Appalachian English. They propose one variable rule to account for both a-prefixing and initial syllable deletion. The linguistic constraints (both grammatical and phonological) can be ordered isomorphically for both processes, but the social constraints cannot. Therefore, both the application and the non-application of the rule can the same social outcome (i.e. social stigma), while its application always has the same linguistic outcome of phonological reduction (cf. Romaine 1979 for further discussion).

12. Sankoff (1973) has reported at least one case of what one might call a 'purely' syntactic variable, i.e. one whose occurrence is conditioned by purely syntactic factors. The variable bai in Tok Pisin, a future tense marker derived from English 'by and by', may be placed variably before or after the subject depending on whether the subject is a NP or a pronoun. Bai is more likely to precede the subject if it is a

pronoun. According to the criteria for syntactic variation I have set up here however, what is needed to make this a case of true syntactic variation would be a discussion of constructions marking time relations with temporal adverbs (cf. p. 24).

13. Vincent (1979:24) has taken the case of the French periphrastic perfects and their cognates somewhat further since he is interested in what light this kind of 'syntactic complementary distribution' sheds on the nature of syntactic reconstruction. In other words, what is the process of grammaticalization by which être and avoir became dissociated from lexical avoir?

14. Actually, the problem for a theory of syntactic variation and reconstruction is the same; namely, how does one identify constructions which alternate? The difficulty for both areas of enquiry is that the same pattern may be expressed by means of non-cognate material, e.g. 'ways of expressing the future' (cf. also p. 19). Vincent (1979:27) has suggested two major prerequisites for a theory of syntactic reconstruction which are relevant here:
1) the establishment of inventories of possible grammaticalization chains (perhaps in the context of a universal theory of semantic and morpho-syntactic features); and
2) the establishment of inventories of possible construction types, e.g. passives, comparatives, conditionals, etc. and their various realizations in the languages of the world. Lightfoot (1979), however, has taken a much more pessimistic view of the feasibility of syntactic reconstruction than Vincent. The former maintains (1979:7-8) that the notion of syntactic correspondence (as in phonetics) makes no sense. Therefore, an analogous view of syntax is incoherent. There is no clear basis, for example, to say that a certain sentence of Old English corresponds to some Middle English one, and no way to claim that a surface structure is mapped by historical rule into another form occurring at a later stage.

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